COMMENTARY

Glyn Daniel: An Obituary

The Editors of ARC asked me to write something 'different' in memory of Glyn Daniel To write 'differently' about a man whose own inimitable editorials in Antiquity defined the obituary writer's art and beggared his successors is a formidable task. I can write only of my own experience of him, and about a man at once so eclectic and yet so consistent, that I can expect to find both agreement and difference in other readers' perceptions of him.

It is difficult to know at what point Glyn emerged from a sea of well-known names and unknown faces when I was an undergraduate. In that first year we were brought up on his books: The Megalith Builders of Western Europe and The First Civilisations -- friendly little blue-backed Pelicans, well-written and within our reach -- we looked ahead to understanding the arcane mysteries of The Prehistoric Chamber Tombs and sampling The Hungry Archaeologist in France and to a recondite appreciation of snails and Calvados, of oysters and claret. Glyn gave us both a digestible synthesis from his own archaeological experience and promised a deliciously indigestible gastronomic foray under expert guidance. We longed to travel with him. He seemed marvellously erudite, witty, warm and sophisticated -- he would introduce us into this Franco-Celtic world -- and indeed he did.

Although at first he seemed to 'belong' to the Johnians -- privileged creatures -- we came at Part II to understand that he knew about us and cared about us. I had practical experience of this: I still have Glyn's letter, sent to me in Australia, asking whether I would like to come back to do research, pulling me out of the threat of an intransigent down-under future and putting me in the position where I can write this obituary. I never felt that female students were less important than men to Glyn and indeed many of us felt adopted as Johnians and part of the same warm structure which the 'Connection' provided. I also saw Glyn's practical kindness to students when he bailed out one of my incipiently alcoholic friends by a mixture of firmness, cash and reward in what I now recognise as an admirably 'tutorial' way. I learnt something from him then which I never quite forgot -- how to be a good patron or sponsor and when to apply the boot or the carrot. I didn't of course realise it at the time but it stays with me twenty years later.

In the fog of boredom induced by many of the lectures of that period Glyn stood out as someone who could keep you awake. As a raconteur, for Welsh hwill and sheer oratory, he could not be bettered. The past, his backward-looking curiosity, came alive for us through his own intense interest. Generations of us learnt from him how to relax as a lecturer, how to speak directly to an audience and how to involve that audience in the story. He didn't gaze out above our heads, nor did he pace the floor, and his clarity and simplicity were in counterbalance to the growing Americanisation of the jargon-laden New Archaeology which began to invade Cambridge just after I graduated.

One of the saddest aspects of that invasion was that it became fashionable to deride Glyn's scholarship and to ignore his real contribution to archaeology. He was concerned with human beings in a personal sense, in the past as in the present, in contrast to the prevalent interest in institutions and groups where the influence of individuals became lost in the generalisations of the social sciences. Like many European prehistorians his work on megaliths had to be substantially altered in the light of C14 dating and he had to agree, which he did willingly, that some of his early conclusions had been 'wrong'. The Young Turks of subsequent generations inevitably found him an easy target, without acknowledging the building blocks he had provided. Moreover his ideas, stated clearly and concisely in simple English, were not valued by later generations accustomed to woolly thought and verbiage. The new professionalism of the seventies which demanded that archaeologists should be Super People -- competent excavators, scientists and social theorists -- bred a generation who wanted their heroes either to excavate in beards and boots or to pontificate in beards and sandals. Glyn, clean-shaven, urbane and debonair, did not fit these images and the value of his scholarly work on the history of our own discipline was buried beneath the values of the New Archaeology, where anecdote and a strong sense of the importance of the individual take second place.

Most if not all older archaeologists risk this devaluation of their work as fashions change, since our view of the past is endlessly mutable. What Glyn produced was a historical framework of reference for British archaeologists which offers both an explanation and a sense of belonging to the emerging discipline within which he worked. We may want to believe that we are scientists but we need to be reminded at the same time that we are human, concerned with the past of humanity and it is this concern that we inherit from Glyn.

Kate Pretty

* * *

Transitional Traditions

(A comment upon the conference "The Origins and Dispersal of Modern Humans", Cambridge, March 1987.)

During the last week of March 1987, the Cambridge Graduate Centre played host to a conference entitled 'The Origins and Dispersal of Modern Humans', organised by Drs Paul Mellars and Chris Stringer. It was a joint conference between both archaeologists and physical anthropologists and a joint effort to solve a problem considered by both parties to be of utmost importance. As an observer I found the conference to be most interesting: as much for what it revealed about about academic debate and discussion, as for any new light it threw upon the discussion.

* * *

For archaeologists, the debate about modern humans, that is those people whom physical anthropologists classify as Homo sapiens sapiens (H. sap. sap.), centres around the so-called Middle-Upper Palaeolithic transition. For the physical anthropologists it centres upon the actual skeletal change itself. Whilst at the outset, therefore, they appear to be dealing with the same problem, there are in fact two; curiously related and yet at the same time very separate.

In an influential article, Mellars set out the characteristics of the Middle-Upper transition as he saw them for the area of south-west France. He noted the change to a blade technology, and the appearance of many more tool types. Bone work appeared, as did art. Populations increased, both in

terms of band numbers and their sizes. Specialised hunting came in with a 'slaughter'. Finally there was the appearance of a new species of hominid (Mellars 1973). Recent reverberations over the past five years (White 1982 and following articles in Current Anthropology) have argued the points, but for the area described the impression still remains.

Europe-orientated researchers, who at this conference comprised the majority, have examined the record from the rest of the relevant world for clarification of the issue. This has principally involved probing south-west Asia, Central Europe and the north and south of Africa (but not the middle).

The records have proven to be different. For example, blade technology was present within the middle stone age of southern Africa, in the form of the Howieson's Port assemblages. Art is lacking from the assemblages of south-west asia. There are other such examples. Despite this, the concept of the Middle-Upper Palaeolithic transition has lived to fight on.

For the physical anthropologists the issue has centred upon the conflict between two competing `models'. One model sees the exodus from Africa of anatomically modern man. Another suggests that there is a marked degree of continuity within local populations. Both models had their supporters at the conference and the battle continued.

The physical anthropological debate principally concerns changes in hominid skull morphology. There are the brow ridges and their placing on the skull; jaw and teeth sizes; zygomatic arches; occipital bunning, frontal flattening, facial prognathism and a whole range of measurable indices.

Surprisingly enough, the nature of the transition depends very much on the variables which are chosen to represent it. The sheer size of the brow ridges shows some degree of continuity over the immediate transitional period (in situ evolution?) whereas their positioning on the skull, and especially whether they met in the middle, shows a marked break.

The controversy was most evident in the spirited session on the dispersal of modern humans into Australasia and Indonesia. Here is the strongest evidence for a continuity from `archaic' Homo sapiens specimens through to the morphologically modern forms as recognised today.

For instance, modern aboriginal populations of Australia and Papua New Guinea, whilst undoubtedly accepted as H. sap. sap., possess many of the features used for the description of the older species. There are the more pronounced brow ridges, the frontal flattening and the facial prognathism. Wolpoff sees this as being sound proof for his ideas of continuity.

The problem appears not so simple when the older skeletal evidence is taken into account. The remains from the key sites of Kow Swamp and Lake Mungo (earlier than Kow Swamp by about 20,000 years) are very different. The Mungo remains are very gracile when compared to the massively robust Kow Swamp remains. Additionally there is the problem of not knowing for sure whether these differences were culturally produced, through skull manipulation during growth, or not.

Into this battle has recently come a whole series of genetic studies. Work on the mitocondrial DNA from modern populations has suggested that there are two separate groups in the modern human population. One comprises modern African populations, the other is the rest of the world. They are both linked further back in time. The interpretation drawn is that this represents a radiation of population out of Africa at some time in the past. We all become related to a single African ancestor.

The geneticists have put the date for this at between 50 to 500 thousand years ago. Their own estimate is 200,000 years. This estimate compares to the earliest finds of H. sap. sap. from Omo at approximately 130,000 years BP. The `Out of Africa' model, as it was christened in the conference, gained new strength.

This, however, makes more of it than can as yet be made. The mitocondrial DNA is not specifically linked to H. sap. sap. and so does not necessarily chart that radiation. The date is, moreover, as the geneticists constantly pointed out, an estimate within a much larger time bracket. The tremendous impact of this work is the result more of the correlations attached to the genetic data, or in other words, the correlations that are made between it and the two current models, than of the evidence itself.

This brings us back to the beginning again. The debate about the origins of modern humans is similar to that of the emergence of domestication. Archaeologists suggested a date for the event. Zoologists formulated their criteria for domesticated species around the changes at this time period. Then archaeologists started to justify their dating of the event by reasoning to the zoological data. There is likewise a continuous interplay between archaeological ideas about the Middle-Upper Palaeolithic transition and anthropological ideas about the change from Homo sapiens Neandertalensis to H. sap. sap.

Unlike the debate about domestication, though, there is much more at stake. In H. sap. sap. we have recognised ourselves and invested our identity. In H. sap. sap. we see the possession of symbolism, the taste for art, the production of fine technologies in various materials which show stylistic investment and development, the formation of larger social groups and finally the ability to hunt, to express our complete mastery over nature. In H. sap. sap. there lies the boundary between 'us' and what went before, and by implication the rest of the animal world.

The 'origins' of fully modern man is a structuralists' dream. It is a wonderful example of boundary behaviour. It sets up rules for inclusion and exclusion. When the debate is looked at, striking features become apparent. The comparisons are always made across the 'transition', across the boundary. This is done, however, by comparing not so much the forms immediately before and after this event, but rather comparing gross characterisations of all that went before with all that came after. Thus the appearance of the Neandertals and all that they made is compared with the appearance of H. sap. sap. and all that they made.

Factors which blur this image are specially accounted for. The Chatelperronian Neandertals of Arcy-sur-Cure copied their blade technology and art from more advanced incoming groups. They themselves remained primitive and died out soon afterwards. The `crude' technologies of Australian aboriginal groups are masked by their impressive art, complex social life and skeletal morphology. The burial symbolism of the Neandertals is quietly forgotten.

That there was some change is not necessarily very difficult to

account for. Foley stressed how easy it was for diseases brought in by contact with new populations to have devastating effects. The recent history of the South American Indians supports this. Zubrow, in a simulation modelling experiment, pointed out that just a small increase in life expectancy would lead to one population replacing another in a time period of less than 10,000 years. Both can therefore accomodate the change without recall to ideas centred upon progress, yet the position still implicitly stands.

What stands in archaeology today is a boundary which has been around for a very long time. In the past, though, it has appeared in different places. There used to be a large debate concerning the uniqueness of humanity. Our uniqueness rested in our ability to make tools, that is until the chimpanzees were known to do it. Rather ridiculously, it then shifted to the ability to use tools for the production of other tools.

In the 19th century, as documented by Gould (1982), boundaries of 'betterness' were set up between white and non-white. Men were separated from women. General characteristics were set up for each. White men were described in terms of the 'nobility' of the European gentry. Non-whites, such as Hottentots and Arabs, were portrayed as primitive and evil. Women, even white European women, were seen as being less intelligent than their male counterparts.

In the same century, the furore over the publication of Darwin's ideas and the implied link between civilised humans and the apes, was the result of a similar boundary, that between humankind and nature.

In the 1960s Neandertals were given a ticket to travel on the New York subway. Now it would be one for the zoo, the inside that is.

It is this boundary, and its importance, which has made the transition to modern humans such an important issue and one which is almost impossible to solve. Like an all powerful deterrent it has kept the status quo for the last twenty years.

Despite there being a faint inkling that the boundary was made more of than was reasonable (by Geoff Clark) and that there was development within the Upper Palaeolithic and the Mousterian, at the end of the conference the feelings of frustration of some indicated that all was very much the same. If future conferences on this topic are to be worthwhile, it is this issue which must be dealt with first of all, not changes in brow ridges or blade technologies.

References

Gould, S.J. 1982. The Mismeasure of Man. London, Penguin.

Mellars, P. 1973. The character of the Middle-Upper Palaeolithic transition in south-west France. In Renfrew, C. (ed.) The Explanation of Culture Change. London, Duckworth.

White, R.H. 1982. Rethinking the Middle-Upper Palaeolithic transition. Current Anthropology 23, 245-276.

Anthony Sinclair

* * * * * *

BOOK REVIEWS

ROBIN TORRENCE, Production and Exchange of Stone Tools. Cambridge University Press, Cambridge, 1986. pp256 (56 figs. and 35 tables). £27.50 (Hard) ISBN 0-521-25266-0.

Reviewed by Richard Bradley

This volume brings together the results of work in two areas of modern archaeology. It is at the same time a contribution to the study of prehistoric exchange systems and an original analysis of a large body of worked stone. The two are brought together in a valuable account of the prehistoric obsidian quarries of Melos.

The author quite rightly regards this study as a contribution to middle-range theory, in the sense employed by Lewis Binford, who supervised the original research. We begin with a thorough review of exchange studies in recent archaeology, and of the difficulties that arise when we apply theoretical models of exchange systems to the distributions of artefacts originating from a single source. It may be possible to pin-point their area of origin, but simulation studies have shown that different types of exchange system may produce the same spatial patterning in the archaeological record. We shall not be able to investigate the relationship between production and exchange unless we can overcome this difficulty. It follows that it may also be impossible to decide whether the production and exchange of particular artefacts took place under centralised political control. In the case of Melos this has a direct bearing on the processes which led to the rise of this 'island polity'.

At the same time, it may be easier to investigate the distribution of worked stone than the movement of other materials, since stoneworking is a subtractive process which leaves easily recognisable (and virtually indestructible) by-products at every stage. Moreover, the fact that stoneworking depends on certain physical constants means that we can employ our knowledge of the raw material, combined with experimentation, to assess the degree of skill invested in the process.

Torrence's approach to the production of obsidian artefacts attempts to break the deadlock in exchange studies through our knowledge of the properties of worked stone. Instead of inferring the nature of these processes from the distribution of the finished artefacts, she uses our knowledge of stoneworking to assess the efficiency with which they were being made at the quarries themselves. The evidence from Melos is compared with a series of ethnographic case studies in which the wider context of stoneworking is already known.

These examples range from the extraction of lithic materials by modern hunter-gatherers to the recent gunflint industry. On this basis she suggests that there is a relationship between the efficiency with which raw materials were extracted and worked, the standardisation of the end product and the degree of specialist involvement in production. Such comparisons also extend to the spatial organisation of the quarry workshops. Applying this approach to the evidence from Melos, she rejects the argument